(preliminary)

Investigating Treatment Effects of Participating Jointly in SNAP and WIC when the Treatment is Validated Only for SNAP[*]

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Abstract

The U.S. Department of Agriculture (USDA) operates several food assistance programs aimed at alleviating food insecurity among low-income households. While many assistance recipients participate in more than one food program, little is known about how the programs interact. The National Household Food Acquisition and Purchase Survey (FoodAPS) provides self-reported information about a household's participation in selected food programs and validates participation status in the Supplemental Nutrition Assistance Program (SNAP), which is the largest program by expenditures. We leverage these key features of FoodAPS to focus on two programs—SNAP and the Special Supplemental Nutrition Program for Women, Infants, and Children (WIC)—and address the following questions:

(1) To what extent does joint participation in SNAP and WIC alleviate food insecurity compared with participation in SNAP or WIC alone?

(2) How can we combine self-reported program participation with administrative SNAP validation data to tighten inference on the causal effects of the programs?

Identifying causal effects in this context presents a major methodological challenge due to two fundamental identification problems: (i) endogenous self-selection of households into the programs and (ii) systematic underreporting of food assistance in national surveys. We extend existing nonparametric treatment effect methods that account for endogenous selection and misreporting in a unifying framework to estimate bounds that isolate true causal effects. We derive several new econometric results to highlight what can be learned about average treatment effects (ATEs) when validation information about participation is available for one program (SNAP) but not the either (WIC). To tighten these bounds, we utilize the Geography Component of FoodAPS (FoodAPS-GC) to construct monotone instrumental variables (MIVs) representing selected aspects of the local food environment, including the availability of food stores and the cost of food. Monotone instrumental variables are weaker than standard IVs in that they require no *a priori* exclusion restriction.

Our key finding is that we can identify the ATE on food security of jointly participating in SNAP and WIC versus participating in SNAP alone as strictly positive under relatively weak assumptions on the selection process combined with a novel food expenditure MIV. Participating in both programs compared with SNAP alone is estimated to increase the food security rate in our sample of low income households by at least 24 percentage points. Accounting for sampling variability reflected in the 95 percent confidence interval, food security would rise by at least 1.9 percentage points. Better understanding of the role played by joint program participation in enhancing food security can be applied to improve the design and targeting of food assistance programs.

JEL codes: C21, I38

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1. Introduction

A household is food secure if it has access to enough food for an active, healthy life of all household members. Food security implies that nutritionally adequate and safe foods are readily available and the household has an assured ability to acquire them in socially acceptable ways (NRC, 2006). Substantial prevalence of food insecurity in the low-income U.S. population is a matter of intense public concern, since food insecurity can be detrimental to the health and well-being of adults and children (for a literature review, see Gundersen et al., 2011). In fact, among households with income below 130% of the federal poverty threshold in 2016, 20.2% experienced low food security and 15.5% had very low food security, implying the food insecurity rate of 35.7% in this population group (Coleman-Jensen et al., 2017a; 2017b).[1]

Since the 1930s, the United States has had several food assistance programs in place designed to alleviate food insecurity. The U.S. Department of Agriculture (USDA) presently administers 15 domestic food programs (Oliveira, 2017). The largest and third largest by total expenditures are, respectively, the Supplemental Nutrition Assistance Program (SNAP; $70.8 billion spent in the fiscal year 2016, 44.2 million participants on average per month) and Special Supplemental Nutrition Program for Women, Infants, and Children (WIC; $5.9 billion, 7.7 million participants). Although target populations and forms of benefits differ across the food programs, their common objective is to provide a nutritionally adequate diet and resources to increase the food supply for low-income households (U.S. GAO, 2010). Yet, 51.2% of households on SNAP and 40.6% on WIC were food insecure in 2016. Moreover, the rate of food insecurity among SNAP recipients was more than twice that among potentially eligible, low-income nonrecipients. In the case of WIC, the food insecurity rate among recipients was 1.4 times that among nonrecipients (Coleman-Jensen et al., 2017a). Such counterintuitive associations motivate a careful investigation of the programs' effectiveness in alleviating food insecurity.

Many food assistance recipients are eligible for and participate in several food programs concurrently, but little is known about how various programs interact in creating a food safety net. Potentially, participation in several programs may reduce the overall resource variability in a household and provide additional mechanisms to address food insecurity (for instance, through

[1] For comparison, the rate of food insecurity among all U.S. households was 12.3% (Coleman-Jensen et al., 2017a).

nutrition education). Also, as suggested by prior research (e.g., Brien and Swann, 2000), programs may reinforce the effects of each other due to synergies. Thus, improving methods to study the effects of joint program participation supports a better understanding of the overall efficacy of the U.S. food safety net.

In this paper, we focus on two specific food assistance programs, SNAP and WIC, which touch the lives of millions of adults and children. We leverage unique features of the National Household Food Acquisition and Purchase Survey (FoodAPS) to address the following questions:

(1) To what extent does participation in both SNAP and WIC lead to an increase in household food security compared with participation in only SNAP or only WIC?

(2) To what extent does combining self-reported survey data on program participation with auxiliary administrative data in FoodAPS on SNAP receipt help tighten inferences on causal average treatment effects (ATEs)?

Identifying causal, rather than associative, effects of a food program is challenging because of (*i*) endogenous self-selection of households into the program (households are not randomly assigned) and (*ii*) pervasive underreporting of food assistance in national surveys. In particular, unobserved household characteristics such as expected future health status, human capital characteristics, and financial stability, for example, are thought to be jointly related to both food security outcomes and the choice to participate in food assistance programs. This simultaneity precludes the use of simple regression techniques (e.g., OLS or probit) to estimate causal effects (see Gundersen and Oliveira, 2001; Jensen, 2002; Fox et al., 2004; Wilde, 2007; Nord and Golla, 2009). Furthermore, households are thought to systematically underreport the receipt of food assistance in surveys (e.g., Bollinger and David, 1997; Meyer et al., 2015a; 2015b), and the propensity to misreport may vary across households based on both observed and unobserved characteristics. For example, comparing survey responses with data from administrative sources, Meyer et al. (2015a) find that less than 60% of SNAP benefits are recorded in recent waves of the Current Population Survey (CPS). Similarly, Bitler et al. (2003) find evidence of “severe” underreporting of WIC benefits. Under such circumstances, all of the classical measurement error assumptions are violated, and it is particularly important to exploit any available validation information to mitigate the measurement error problem. Analyzing the effects of not just one, but two food programs at once adds another layer of complexity. We must devise an approach to model food program participation that is reflective of the participation's intensity, but does not impose a rigid structure on the program effects. Also, we must address the increased dimensionality of the measurement error problem.

This paper develops a new framework for assessing the causal effects of participation in up to two programs when participation can be partially verified. In particular, we extend the nonparametric treatment effect methods developed by Kreider and Hill (2009) and Kreider, Pepper, Gundersen, and Jolliffe (2012, hereafter KPGJ) to account for the endogenous selection and nonclassical measurement error identification problems in a unifying framework. We modify these partial identification methods, which are designed to handle a binary treatment variable, to accommodate a multinomial, partially-ordered treatment variable. We also extend the methodology in order to exploit partial validation of the treatment variable (FoodAPS provides both self-reported and administratively verified SNAP receipt data, but only self-reported WIC data). This approach allows us to incorporate auxiliary administrative information to tighten inferences on the causal effects of program participation and to assess broader impacts of misreporting when such information is unavailable. Our methodology significantly differs from the approaches of Fraker and Moffitt (1988), Keane and Moffitt (1998), and Brien and Swann (2001) in which participation decisions and the effects of programs on an outcome of interest are modeled jointly in a parametric, simultaneous equations setting.[2] Notably, our methods do not impose the linear response assumption or any of the classical measurement error assumptions.

An important component of our analysis involves investigating the power of assumptions in tightening inferences on causal effects. For example, we investigate a number of middle ground assumptions aimed at narrowing worst-case bounds under endogenous selection by restricting relationships between food security, participation in SNAP and WIC, and observed covariates. In particular, the Monotone Treatment Selection (MTS) assumption (Manski and Pepper, 2000) formalizes the idea that the decision to participate is monotonically related to food security outcomes: households participating in the programs possess attributes that are detrimental to food security and, therefore, are presumed to have worse latent food security outcomes on average. This assumption, which links households' characteristics to the propensity to be food secure, is distinct from a Monotone Treatment Response (MTR) assumption (Manski, 1997) that directly links program

[2] Also, in contrast to our paper, these studies do not address misreporting.

participation to food security. Under the MTR assumption, more participation cannot harm food security on average. A Monotone Instrumental Variable (MIV) assumption (Manski and Pepper, 2000) formalizes the notion that the latent probability of food security varies monotonically with certain observed covariates. Specifically, we study the identifying power of an assumption that, on average, latent food security weakly rises with more household food expenditures relative to expenditures consistent with the Thrifty Food Plan (Carlson et al., 2007) and an assumption that it weakly rises with household income adjusted for household composition. Unlike standard instrumental variables (IVs), MIVs require no a priori exclusion restrictions (or mean independence assumptions). In addition, we investigate the identifying power of assumptions related to the misreporting process (e.g., no false positives) as well as of the (implausible) exogenous selection assumption and of IVs pertaining to state-level SNAP policies and design features, which are extracted from the SNAP Policy Database (ERS, 2017b).

FoodAPS, which is our main data source, contains records for 4,826 households, who participated in the survey during one week between April 2012 and January 2013. We focus only on those that would be eligible to participate in SNAP and WIC concurrently. Given the eligibility restrictions associated with the two food programs, we choose to analyze households with income below 130% of the poverty threshold and containing a pregnant woman, or a child aged less than five years. The analytical sample contains 460 households, 37% of whom report being on both programs. Although bounds on ATEs tend to be wide without assumptions, by layering successively stronger assumptions, we are able to provide successively tighter bounds on the food program effects. Our key finding is that we can identify the ATE of jointly participating in SNAP and WIC versus participating in SNAP alone as strictly positive under relatively weak assumptions on the selection process combined with a food expenditure MIV.

The remainder of the paper is organized as follows. Section 2 lays out the methodological framework, formally defines the identification problems, and provides several new sets of closed-form analytical formulas for bounding ATEs given a potentially mismeasured, partially ordered, and partially verified treatment. We describe empirical results corresponding to the new theory, highlighting the identifying power of successively stronger nonparametric assumptions. Section 3 describes the data and outlines the characteristics of the analytical sample. Section 4 provides additional sensitivity analysis, and Section 5 concludes.

2. Methodology

2.1. General framework

To estimate causal treatment effects of multiple program participation, we apply nonparametric bounding methods developed by KPGJ that account for the dual identification problems of endogenous selection and classification error.[3] We extend their framework to introduce partially ordered multiple treatments, which is necessary to model joint program participation. We derive sharp bounds on average treatment effects that are logically consistent with the observed data and imposed statistical and behavioral assumptions. As described below, we also exploit a key feature of FoodAPS in that it contains both self-reported and administratively verified data on SNAP participation. This verification information is valuable in part because it allows us to reduce the dimensionality of the classification error problem.

For the outcome, let $Y = 1$ indicate that a household is food secure, with $Y = 0$ otherwise. We also consider related outcomes such as very low food security. Let S^* be an unobserved indicator of true program participation where $S^* = 0$ denotes no participation in SNAP or WIC, $S^* = 1$ denotes participation in SNAP alone, $S^* = 2$ denotes participation in WIC alone, and $S^* = 3$ denotes participation in both SNAP and WIC. This treatment variable is partially ordered: $S^* = 1$ or 2 denotes some participation while $S^* = 0$ does not, and $S^* = 3$ involves more participation. (Since $S^* = 1$ and $S^* = 2$ represent different programs, these two treatments are not ordered.)

Instead of observing S^* in the data, we observe a self-reported counterpart, S. We also observe FoodAPS administrative information on SNAP. Let $V_{SNAP} = 1$ denote verification that a household truly received SNAP (weighted 57.6% of the analytical sample, using the FoodAPS variable "*snapnowadmin*"), implying that $S^* = 1 \text{ or } 3,$ with $V_{SNAP} = 0$ (42.4% of the sample) conversely implying that $S^* = 0 \text{ or } 2.$[4]

We focus ATEs associated with participating in both food assistance programs versus a

[3] See also Gundersen et al. (2012).

[4] Households with $V_{SNAP} = 0$ include verified SNAP non-participants (weighted 2.6% of the analytical sample), households that could not be matched to existing administrative records (26.8%; most likely due to true nonparticipation), households that could not be matched because states provided no or insufficient administrative data (10.7%), and households that withheld consent for the administrative match (2.3%). For simplicity, we treat all of these households as true nonparticipants.

single program, or compared with no participation:

$$ATE_{jk} = P[Y(S^* = j) = 1 \mid X] - P[Y(S^* = k) = 1 \mid X] \text{ for } j,k \in \{0,1,2,3\},\ j \neq k, \quad (1)$$

where $Y(S^*)$ indicates the (latent) potential food security outcome under treatment S^*, X denotes any covariates of interest, and P denotes the probability of an outcome.[5] It should be noted that there are no regression orthogonality conditions to be satisfied in our framework; thus, there is no need to include covariates as a means of avoiding omitted variable bias. To simplify notation, we suppress the conditioning on X and write $P[Y(S^* = j) = 1]$ more compactly as $P[Y(j) = 1]$.

In what follows, we illustrate the case of $j = 3$ vs. $k = 1$. Here, $ATE_{31} = P[Y(3) = 1] - P[Y(1) = 1]$ measures how the prevalence of food security would change if all eligible households participated in both SNAP and WIC rather than in SNAP alone.[6] One cannot identify ATE_{31} without additional assumptions, even if S is accurately reported, because the potential outcome $Y(S^* = 3)$ is observed only for households that chose to participate in both SNAP and WIC, while $Y(S^* = 1)$ is observed only for households that chose to participate in SNAP alone. The decomposition $P[Y(3) = 1] = P[Y(3) = 1 \mid S^* = 3]P(S^* = 3) + P[Y(3) = 1 \mid S^* \neq 3]P(S^* \neq 3)$ highlights the selection problem: the term $P[Y(3) = 1 \mid S^* \neq 3]$ represents an unobserved counterfactual outcome, namely, the likelihood of food security when participating in SNAP and WIC concurrently among households that actually chose not to be on both programs.

As a further identification problem, households are thought to systematically underreport program participation in national surveys, and such misreporting may be related to personal characteristics (including the food security outcome itself). Allowing S to deviate from S^*, let $\theta_i^{j,k} \equiv P(Y = i, S = j, S^* = k)$ for $j,k = \{0,1,2,3\}$ denote the fraction of households with food security status i reporting participation status j when true participation status is k. Using the law of total probability, the first term in ATE_{31} becomes $P[Y(3) = 1] = P(Y = 1, S = 3) + \theta_1^{-3,3} - \theta_1^{3,-3}$

[5] Our framework can be extended to handle continuous outcomes or the number of affirmed food insecurity conditions in the survey.

[6] Note that we are not restricting a treatment effect to be the same across different households. As emphasized by Moffitt (2005), the classical linear response model assumption, for example, is difficult to justify when considering government assistance programs that are thought to have heterogeneous effects.

$+P[Y(3)=1 \mid S^* \neq 3]\left\{P(S \neq 3)+\sum_{j \neq 3}(\theta_1^{-j,j}+\theta_0^{-j,j}-\theta_1^{j,-j}-\theta_0^{j,-j})\right\}$, where $\theta_i^{-j,k} \equiv P(Y=i, S \neq j, S^*=k)$ and $\theta_i^{j,-k} \equiv P(Y=i, S=j, S^* \neq k)$. An analogous expression can be derived for $P[Y(1)=1]$.

Without further assumptions, we show in Appendix A that the marginal impact on food security associated with participating in both SNAP and WIC, compared with participating in SNAP alone, is bounded as follows:[7]

$$\begin{gathered} -1+P(Y=1,S=3)+P(Y=0,S=1)+\Theta_{3,1}^{LB} \\ \leq ATE_{3,1} \leq \\ 1-P(Y=0,S=3)-P(Y=1,S=1)+\Theta_{3,1}^{UB} \end{gathered} \tag{2}$$

where $\Theta_{3,1}^{LB} \equiv \theta_1^{-3,3}-\theta_1^{3,-3}+\theta_0^{-1,1}-\theta_0^{1,-1}$ and $\Theta_{3,1}^{UB} \equiv -\theta_0^{-3,3}+\theta_0^{3,-3}-\theta_1^{-1,1}+\theta_1^{1,-1}$ could be positive or negative. Terms like $P(Y=1,S=3)$ are observed from the data, but the $\{\theta\}$ components are unobserved. Thus, the ATE bounds in Equation (2) are not yet operational. In our FoodAPS sample, we have $P(Y=1,S=3)=0.238$, $P(Y=0,S=1)=0.159$, $P(Y=0,S=3)=0.165$, and $P(Y=1,S=1)=0.172$. Thus, in our application the bounds in Equation (2) become

$$-0.603+\Theta_{3,1}^{LB} \quad \leq ATE_{3,1} \leq \; 0.663+\Theta_{3,1}^{UB}$$

If participation in SNAP and WIC were accurately measured, then setting $\Theta_{3,1}^{LB}$ and $\Theta_{3,1}^{UB}$ equal to zero would reduce the bounds in Equation (2) to Manski's (1995) classic worst-case ATE bounds: $[-0.603, 0.663]$.[8] In the context of food assistance programs, however, participation is thought to be underreported.

Importantly, these error rates $\Theta_{3,1}^{LB}$ and $\Theta_{3,1}^{UB}$ are logically bounded. For example, $\theta_1^{-1,1}$ cannot exceed $P(Y=1,S\neq 1)=0.378$, a quantity directly observed in the data. Without knowledge about the nature

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[7] To do: add this result to the proofs in Appendix A and use the following information: For the lower bound, $-P(Y=0,S\neq 1)-P(Y=1,S\neq 3) = -1+P(Y=0)+P(Y=1) - P(Y=0,S\neq 1) - P(Y=1,S\neq 3)$ $= -1+P(Y=0,S=1)+P(Y=1,S=3)$. For the upper bound, $P(Y=0,S\neq 3)+P(Y=1,S\neq 1)$ $= 1-P(Y=0)-P(Y=1) + P(Y=0,S\neq 3) + P(Y=1,S\neq 1) = 1-P(Y=0,S=3)-P(Y=1,S=1)$.

[8] With a binary treatment, the Manski bounds would have a width equal to 1 (and always include 0). In the present context with multiple treatments, the Manski bounds have a width greater than 1.

and degree of reporting errors, however, nothing prevents the worst case bounds in Equation (2) from expanding to $[-1,1]$, in which case they are completely uninformative. For the upper bound, for example, $\theta_0^{3,-3}$ could be as large as $P(Y=0,S=3)=0.165$, while $\theta_1^{1,-1}$ could be as large as $P(Y=1,S=1)=0.172$. Since $\theta_1^{1,-1}$ and $\theta_1^{1,-1}$ could both be 0, the upper bound in Equation (2) attains 1. Analogously, the lower bound attains -1.

2.2. Partial validation data in FoodAPS

Partial validation data in FoodAPS allow us to place informative restrictions on the magnitudes of $\Theta_{3,1}^{LB}$ and $\Theta_{3,1}^{UB}$. Knowledge of whether or not a household participates in SNAP is not enough to pinpoint the value of S^*, which represents the true joint participation status. In particular, confirmation of participation in SNAP merely identifies that $S^* \in \{1,3\}$; that is, the household might be participating in SNAP alone or in both SNAP and WIC. Similarly, confirmation of nonparticipation in SNAP merely identifies that $S^* \in \{0,2\}$; the household may have been participating in neither program or in WIC alone. Still, confirmation of SNAP participation status – and modifying the observed treatment indicator *S* accordingly to align with known values – allows us to eliminate many of the error components of $\Theta_{3,1}^{LB}$ and $\Theta_{3,1}^{UB}$. Specifically, $\Theta_{3,1}^{LB} \equiv (\theta_1^{0,3}+\theta_1^{1,3}+\theta_1^{2,3})-(\theta_1^{3,0}+\theta_1^{3,1}+\theta_1^{3,2})+(\theta_0^{0,1}+\theta_0^{2,1}+\theta_0^{3,1})-(\theta_0^{1,0}+\theta_0^{1,2}+\theta_0^{1,3})$ reduces to $\Theta_{3,1}^{LB}=\theta_1^{1,3}-\theta_1^{3,1}+\theta_0^{3,1}-\theta_0^{1,3}$ after setting $\theta_1^{0,3}=\theta_1^{2,3}=\theta_1^{3,0}=\theta_1^{3,2}=\theta_0^{0,1}=\theta_0^{2,1}=\theta_0^{1,0}=\theta_0^{1,2}=0$. Eight of the 12 error components vanish using the FoodAPS validation information. For example, $\theta_1^{0,3} \equiv P(Y=1,S=0,S^*=3)=0$ since SNAP validation rules out cases in which a household ends up falsely classified as participating in neither program since we have documentation that the household participated at least in SNAP. Similarly, $\Theta_{3,1}^{UB} \equiv -(\theta_0^{0,3}+\theta_0^{1,3}+\theta_0^{2,3})+(\theta_0^{3,0}+\theta_0^{3,1}+\theta_0^{3,2})$ $-(\theta_1^{0,1}+\theta_1^{2,1}+\theta_1^{3,1})+(\theta_1^{1,0}+\theta_1^{1,2}+\theta_1^{1,3})$ reduces to $\Theta_{3,1}^{UB}=-\theta_0^{1,3}+\theta_0^{3,1}-\theta_1^{3,1}+\theta_1^{1,3}$ after setting $\theta_0^{0,3}=\theta_0^{2,3}$ $=\theta_0^{3,0}=\theta_0^{3,2}=\theta_1^{0,1}=\theta_1^{2,1}=\theta_1^{1,0}=\theta_1^{1,2}=0$.

Using the FoodAPS validation data, the average treatment effect bounds in Equation (2) are thus narrowed as follows:

$$-1+P(Y=1,S=3)+P(Y=0,S=1)+\theta_1^{1,3}-\theta_1^{3,1}+\theta_0^{3,1}-\theta_0^{1,3}$$
$$\leq ATE_{3,1} \leq \tag{3}$$
$$1-P(Y=0,S=3)-P(Y=1,S=1)+\theta_1^{1,3}-\theta_1^{3,1}+\theta_0^{3,1}-\theta_0^{1,3}.$$

Note that the error components $\theta_1^{1,3}-\theta_1^{3,1}+\theta_0^{3,1}-\theta_0^{1,3}$ shift the lower and upper bound by the same unknown constant. In our application,

$$-0.603+\theta_1^{1,3}-\theta_1^{3,1}+\theta_0^{3,1}-\theta_0^{1,3} \quad \leq ATE_{3,1} \leq \quad 0.663+\theta_1^{1,3}-\theta_1^{3,1}+\theta_0^{3,1}-\theta_0^{1,3}.$$

Despite eliminating eight of the 12 error components in $\Theta_{3,1}^{LB}$ and $\Theta_{3,1}^{UB}$, however, the bounds in Equation (3) are still completely uninformative: the ATE may still lie anywhere between -1 and 1. To see this, it is instructive to understand why the bounds in Equation (3) are informative in the absence of measurement error. In that case, the lower bound is elevated above -1 because some fraction of households $P(Y=1,S=3)=0.238$ are known to be food secure while participating in both SNAP and WIC, while another fraction $P(Y=0,S=1)=0.159$ are known to be food insecure while participating in SNAP alone. The presence of these groups reveals that, at least sometimes, participation in both programs is not harmful relative to participation in SNAP alone. Similarly, the upper bound cannot attain 1 when some fraction of households $P(Y=0,S=3)=0.165$ are known to be food insecure despite participating in both programs, and some from fraction $P(Y=1,S=1)=0.172$ are food secure despite participating only in SNAP. Thus, at least sometimes, participation in both programs is not beneficial compared with participation in SNAP alone.

In the presence of classification error, the difficulty is that $\theta_1^{3,1}=P(Y=1,S=3,S^*=1)$ in the lower bound could be as large as $P(Y=1,S=3)=0.238$ while $\theta_0^{1,3}=P(Y=0,S=1,S^*=3)$ could be as large as $P(Y=0,S=1)=0.159$. Without further assumptions to constrain the patterns or degrees of misclassification, logically we cannot rule out the possibility that food secure households claiming to participate in both programs were actually participating only in SNAP. Nor can we rule out the possibility that food insecure households claiming to participate in SNAP alone were actually participating in both programs. The lower bound falls back to -1, returning to being completely uninformative. Similarly, the upper bound rises back to 1. While these scenarios are obviously extreme, they help crystalize how data must be combined with assumptions before we can make logical, informative inferences.

FoodAPS currently does not contain information that can be used to validate WIC participation status.[9] Thus, we cannot further constrain the error components in $\Theta_{3,1}^{LB}$ or $\Theta_{3,1}^{UB}$ using data alone. Apart from not knowing whether a household is truly on WIC, we also do not know whether a household verified to be participating in SNAP was on SNAP alone $(S^*=1)$ or participating in both programs $(S^*=3)$. To learn anything about $ATE_{3,1}$, we need to impose assumptions about the magnitudes or patterns of reporting errors. Our objective is to strike a reasonable balance between making assumptions weak enough to be credible while strong enough to remain informative.

2.3. No false positives

Combined with the FoodAPS validation data for SNAP participation, we can make further progress in bounding $ATE_{3,1}$ by imposing a common "no false positives" assumption in the food assistance literature (e.g., Almada et al. 2016, KPGJ) that households do not falsely report benefits they do not actually receive. Validation data from previous studies find only rare instances of these errors of commission (e.g., Bollinger and David 1997; Marquis and Moore 1990). In our FoodAPS sample, only 1.8% of those reporting SNAP benefits were found not to be receiving benefits. Under the no false positives assumption, the ATE bounds in Equation (3) reduce further and now become informative:[10]

$$\begin{aligned}&-1+P(Y=1,S=3)+P(Y=0,S=1)+\theta_1^{1,3}-\theta_0^{1,3}\\&\leq ATE_{3,1}\leq \\&1-P(Y=0,S=3)-P(Y=1,S=1)+\theta_1^{1,3}-\theta_0^{1,3}.\end{aligned} \tag{4}$$

[9] There is information reported on food expenditures funded through WIC vouchers at purchase events that might be used to partially validate participation. We plan to investigate this as a source of additional information. Of concern is the potential for lags in the timing of holding WIC benefits after no longer being considered a participant.
[10] Alternatively, or in addition, one could impose an upper bound on the degree of data corruption in the spirit of Horowitz and Manski (1995). For example, one could impose a constraint that no more than some known percentage of households misreport their participation status.

Note that the error components $\theta_1^{1,3} - \theta_0^{1,3}$ shift the lower and upper bound by the same unknown constant. Taking worst cases across $\theta_1^{1,3}$ and $\theta_0^{1,3}$, we can sharply bound $ATE_{3,1}$ as follows:

Proposition 1A. *Given verification of SNAP status but not WIC status, the impact on food security associated with participating in both programs compared with SNAP alone is sharply bounded as follows:*

$$-1 + P(Y=1, S=3) \leq ATE_{3,1} \leq 1 - P(Y=0, S=3).$$

See Appendix A for a proof.

These bounds are very wide, with width $2 - P(Y=0, S=3) - P(Y=1, S=3)$. Using our FoodAPS sample, $ATE_{3,1}$ may lie anywhere in the range $[-0.762, 0.835]$ with a width of 1.60. We have made important progress, however, in moving away from the [-1, 1] no-information bounds. Specifically, a fraction of households $P(Y=1, S=3) = 0.238$ are food secure while claiming to participate in both programs, thus raising the lower bound away from -1. We trust their participation responses under the no false positives assumption. Similarly, a fraction of households $P(Y=0, S=3) = 0.165$ are food insecure despite participating in both programs, thus lowering the upper bound away from 1.

To gain an understanding of how misreporting affects uncertainty about $ATE_{3,1}^{WC}$ beyond uncertainty created by unknown counterfactuals, we trace out the Equation (4) bounds as a function of $\theta_1^{1,3}$ and $\theta_0^{1,3}$ in Figure __ (not yet available).

One way to narrow the Proposition 1 bounds is to further restrict the nature of classification errors. Suppose, for example, that misreporting of SNAP or WIC participation arises independently of the household's food security status. This nondifferential errors assumption specifies that $P(S^* = j \mid S = k, Y = 1) = P(S^* = j \mid S = k, Y = 0)$. Evidence from FoodAPS suggests that food secure and food insecure households are about equally likely to misreport the receipt of food assistance.[11] In this case, we can write $\theta_0^{1,3} = \kappa \theta_1^{1,3}$ in Equation (4), where $\kappa \equiv P(Y=0, S=1) / P(Y=1, S=1)$ is

[11] In The chances of being found to participate in SNAP when claiming otherwise is about 49% among food secure households and 44% among food insecure households. The fractions are also similar for the rare cases of reporting SNAP benefits not actually received.

observed in the data.[12]

$$
\begin{aligned}
-1+P(Y=1,S=3)+P(Y=0,S=1)+(1-\kappa)\theta_1^{1,3} \\
\le ATE_{3,1} \le \qquad\qquad (5) \\
1-P(Y=0,S=3)-P(Y=1,S=1)+(1-\kappa)\theta_1^{1,3}
\end{aligned}
$$

This assumption has substantial identifying power, especially when κ is close to 1. When $\kappa = 1$ such that half the households reporting participation in SNAP alone are food secure, with the other half food insecure, classification error ceases to be an issue. In that case, the worst-case bounds in Equation (5) reduce to Manski's (1995) classic worst-case bounds described above. Otherwise, the bounds reduce to the following:

Proposition 1B. *Under the nondifferential errors assumption* $P(S^* = j \mid S = k, Y = 1)$ $= P(S^* = j \mid S = k, Y = 0)$ *that participation errors arise independently of food security status, the Proposition 1 bounds narrow as follows:*

$$
\begin{aligned}
-1+P(Y=1,S=3)+\min\left\{P(Y=0,S=1),P(Y=1,S=1)\right\} \\
\le ATE_{3,1} \le \\
1-P(Y=0,S=3)-\min\left\{P(Y=0,S=1),P(Y=1,S=1)\right\}
\end{aligned}
$$

See Appendix A for a proof.

Notice the similarity between these Proposition 1B bounds and Manski's worst-case bounds in Equation (2) when there is no measurement error $(\Theta_{3,1}^{LB} = \Theta_{3,1}^{UB})$. In the reference case that $\kappa = 1$, the bounds are identical: SNAP verification combined with no false positives and nondifferential errors is equivalent to assuming no measurement error at all. When $\kappa > 1$ such that more than half of the households reporting participation in SNAP alone are food insecure, the Proposition 1B upper bound is identical to Manski's no-errors upper bound. When $\kappa < 1$ such that more than half of the households reporting participation in SNAP alone are food secure, the Proposition 1B lower bound is identical to Manski's no-errors lower bound.

[12] Specifically, $\theta_0^{1,3} \equiv P(Y=0,S=1,S^*=3) = P(S^*=3 \mid Y=0,S=1)P(Y=0,S=1) = P(S^*=3 \mid Y=1,S=1)P(Y=0,S=1)$ $= \theta_1^{1,3}P(Y=0,S=1)/P(Y=1,S=1) = \theta_1^{1,3}P(Y=0 \mid S=1)/P(Y=1 \mid S=1)$.

In our FoodAPS sample, $\kappa = 0.92 < 1$ which implies $P(Y=0,S=1) < P(Y=1,S=1)$. Thus, the Proposition 2 bounds reduce to

$$\begin{gathered} -1 + P(Y=1,S=3) + P(Y=0,S=1) \\ \leq ATE_{3,1} \leq \\ 1 - P(Y=0,S=3) - P(Y=0,S=1). \end{gathered} \tag{6}$$

In our sample, the Proposition 1A bounds narrow from $[-0.762, 0.835]$ to $[-0.603, 0.676]$ with a width of 1.28, a 32 percentage point reduction in the width. The lower bound of -0.603 is identical to Manski's lower bound reported above.

To vividly demonstrate the impact of assumptions on inference, we next investigate the identifying power of exogenous selection:

$$P[Y(j)=1] = P[Y(j)=1 \mid S^* = k] \quad \forall j,k. \tag{7}$$

Equation (7) means that, on average, potential outcomes do not depend on the realized treatment. The assumption of exogenous selection makes sense when households are assigned to programs in a truly random manner (so that there are no systematic differences in household attributes across different treatment groups). Because households select into food programs on their own accord, exogeneity is unlikely to hold in our setting. Nevertheless, it remains instructive to understand its identifying power:

Proposition 2A. *Under exogenous selection (e.g., random assignment), the Proposition 1A worst-case bounds narrow as follows:*

$$\begin{gathered} -\frac{P(Y=0,S=3) + P(Y=0,S=1)}{P(S=3) + P(Y=0,S=1)} \\ \leq ATE_{3,1} \leq \\ \frac{P(Y=1,S=3) + P(Y=1,S=1)}{P(S=3) + P(Y=1,S=1)} \end{gathered}$$

See Appendix A for a proof.

In our application, the Proposition 1A worst-case bounds narrow from $[-0.762, 0.835]$ to

$[-0.576, 0.713]$. Even though the exogeneity assumption completely eliminates uncertainty associated with unknown counterfactuals – the *only* source of identification uncertainty in most treatment effects models – the bounds remain very wide because of the measurement error problem. As before with Proposition 1, we can further narrow the bounds by imposing the nondifferential errors assumption:

Proposition 2B. *Under exogenous selection combined with nondifferential errors, the Proposition 2A bounds narrow as follows:*

$$P(Y=1 \mid S=3) - P(Y=1 \mid S=1)$$
$$\leq ATE_{3,1} \leq$$
$$\frac{P(Y=1, S=3) + P(Y=1, S=1)}{P(S=3) + P(S=1)}$$

See Appendix A for a proof.

The Proposition 2B lower bound is simply the observed difference in food security rates between households reporting participation in both programs vs. those reporting participation in SNAP alone. In our application, the lower bound is given by $0.5905 - 0.5204 \;\; = 0.0701$. This lower bound is identical to the average treatment effect that would be obtained under conditions of random assignment and no measurement error. Comparing denominators, the Proposition 2B upper bound improves on the Proposition 2A upper bound as long as some households that report participation in SNAP alone are food insecure: $P(S=1) > P(Y=1, S=1)$. In our application, the Proposition 2A upper bound improves from 0.713 to 0.559. (In a subsequent draft, we will add a figure that traces out $ATE_{3,1} = \frac{P(Y=1, S=3) + \theta_1^{1,3}}{P(S=3) + \theta_1^{1,3} + \theta_0^{1,3}} - \frac{P(Y=1, S=1) - \theta_1^{1,3}}{P(S=1) - \theta_1^{1,3} - \theta_0^{1,3}}$ as a function of the two error components.)

Monotonicity assumptions

For the remainder of the analysis, we do not impose the exogenous selection assumption. Instead, we study how the Proposition 1A and 1B worst-case bounds can be narrowed under relatively weak monotonicity restrictions such as Monotone Treatment Selection (Manski and Pepper, 2000; KPGJ) and Monotone Treatment Response (Manski, 1997; KPGJ). The MTS assumption formalizes the notion that unobserved factors related to food insecurity are likely to be positively associated with the decision to take up food assistance programs. Under the MTR assumption, participating in SNAP and WIC would not harm food security, on average, conditional on treatment selection, reflecting a general consensus that food assistance would not cause food insecurity (Currie, 2003).

Formally, the MTS assumption in our partially-ordered treatment framework is specified as follows:

$$P[Y(j)=1 \mid S^*=3] \leq P[Y(j)=1 \mid S^*=k] \leq P[Y(j)=1 \mid S^*=0] \quad \forall\, j \text{ and } k=1,2. \tag{8}$$

For each potential treatment *j*, we posit that the latent food security probability is (weakly) less favorable among households that enrolled in both programs $(S^*=3)$ compared with only one program $(S^*=1 \text{ or } 2),$ and similarly less favorable among households that enrolled in one program compared with no program $(S^*=0).$ We impose no ordering between households that enroll in only one program versus the other. The MTS assumption does not imply that any households would be better off changing their participation status—only that those who chose to participate in more programs start out relatively disadvantaged, on average, under any potential treatment. We prove the following result:

Proposition 3A. *Under the MTS assumption in Equation (7), the Proposition 1 worst-case lower bound improves as follows, with the upper bound remaining unchanged:*

$$ATE_{3,1}^{MTS} \geq -1 + \frac{P(Y=1, S=3)}{P(S=3)+P(Y=0, S=1)}.$$

See Appendix A for a proof.

Using the FoodAPS data, the Proposition 1A bounds improve from $[-0.762, 0.835]$ to $[-0.576, 0.835].$ This lower bound is improved further under the nondifferential errors assumption that misreporting does not depend on food security status:

Proposition 3B. *Under nondifferential errors, the Proposition 3A MTS lower bound improves as follows:*

$$ATE_{3,1}^{MTS} \geq -1 + \max\left\{P(Y=1 \mid S=3), \frac{P(Y=1, S=3) + P(Y=1, S=1)}{P(Y=3) + P(Y=1)}\right\} + P(Y=0 \mid S=1)\left[P(S=3) + P(S=1)\right].$$

See Appendix A for a proof.

In our application, the improvement is dramatic. The Proposition 3A bounds narrow from $[-0.576, 0.835]$ to $[-0.058, 0.835]$. The lower bound is improved 70 percentage points compared with the Proposition 1A worst-case lower bound, and it is improved 54 percentage points compared with Manski's no-errors worst-case lower bound.

Monotone Treatment Response

To formally specify the MTR assumption, we extend Manski's (1995, 1997) original approach. For a given realized program participation status, we suppose that potential participation in SNAP alone or WIC alone would not harm a household's food security on average compared with no participation, nor would participation in both programs be detrimental on average compared with participation in either program alone:

$$P[Y(3)=1 \mid S^*] \geq P[Y(1)=1 \mid S^*] \geq P[Y(0)=1 \mid S^*]$$

$$P[Y(3)=1 \mid S^*] \geq P[Y(2)=1 \mid S^*] \geq P[Y(0)=1 \mid S^*]. \qquad (9)$$

MTR implies $ATE_{3,1}$ is nonnegative, but it does not rule out zero effects. In isolation, this assumption is not informative since it precludes strictly negative effects by construction. It can have identifying power, however, when combined with the instrumental variable assumptions described next. In particular, it assures that the effect is nonnegative across all values of the instrument. We can narrow the bounds further by employing MIVs. Monotone instruments are often easier to motivate than standard IVs, because they do not require any orthogonality/exclusion restrictions. In the application, we merely require that the instrument leads to a weakly improved latent food security outcome, on average, conditional on the treatment. As MIVs, we use variables reflective of important aspects of local food environment, as recorded in FoodAPS-GC.[13] For example, we employ the densities of supermarkets and of all food stores in the household's neighborhood and the ratio of actual household expenditures on food at home to food expenditures consistent with the TFP recommendations and local prices. We also investigate the usefulness of other MIVs, including a conventional MIV based on household income and composition. An assumption underlying these monotone instruments is that, broadly speaking, more resources in the household and easier access to food cannot harm food security.

Let u represent a monotone instrument. The MIV assumption specifies that higher values of u lead to weakly improved food security outcomes, on average, under each treatment:

$$u_1 \le u \le u_2 \Rightarrow P[Y(j)=1 \mid v=u_1] \le P[Y(j)=1 \mid v=u] \le P[Y(j)=1 \mid v=u_2] \text{ for each } j. \quad (5)$$

While these conditional probabilities are not identified, they can be bounded as described by Manski and Pepper (2000). Bounds on the unconditional latent probability, $P[Y(j)=1]$, can, in turn, be obtained by applying the law of total probability and calculating a weighted average of the bounds on $P[Y(j)=1 \mid v=u]$ over different values of u.[14] When combined with MTS or MTR, those assumptions are assumed to apply at each value of the instrument, v.

In our empirical analysis, we layer successively stronger combinations of assumptions in order to investigate how they shape inference and also provide successively tighter bounds on the causal impact of participation in SNAP and WIC. The table below demonstrates the identifying power of the MTS, MTR, and MIV assumptions. Point estimates (p.e.) of the bounds are provided along with Imbens-Manski (2004) confidence intervals that cover the true value of the ATE with 95% probability. Strictly positive estimated average treatment effects are highlighted in bold. The key finding is that we can identify the ATE as strictly positive and statistically significant when combining the MTS, MTR, and expenditure MIV assumptions. Participating in both SNAP and WIC

[13] Previous studies have shown that the local food environment is an important contributor to food security and health through differences in access, availability, and cost of food (e.g., Rose and Richards, 2004; Ver Ploeg, 2010; Bonanno and Goetz, 2012; Lee, 2012). In particular, the relative cost of food in the area can substantially affect a low-income household's ability to provide an adequate diet to its members. Zhylyevskyy et al. (2013) find that lower relative fruit and vegetable prices positively affect the selection of these foods in a study of African American youths and parents.

[14] As noted by Manski and Pepper (2000), the MIV estimator is consistent but biased in finite samples. We employ Kreider and Pepper's (2007) modified MIV estimator that accounts for the finite sample bias using a nonparametric bootstrap correction method. Following KPGJ, we assume that the ratio of actual to potential underreporting does not vary across MIV groups (defined according to the value of *u*).

compared with participating in SNAP alone is estimated to increase the food security rate among low-income households in our sample by at least 24 percentage points. Accounting for sampling variability reflected in the confidence interval, food security would rise by at least 1.9 percentage points. (We will add further discussion.)

Table 1a. ATE Associated with Participating in Both SNAP and WIC vs. Participating in SNAP Alone

(A) Income-to-Poverty MIV:

	Differential Errors		Nondifferential Errors	
	MTS + MIV		**MTS + MIV**	
	LB UB	width	LB UB	width
p.e.	[-0.549, 0.657]	1.206	[**0.0251, 0.657**]	0.632
CI	[-0.694, 0.752]		[-0.143, 0.752]	
	MTR + MIV		**MTR + MIV**	
	LB UB	width	LB UB	width
p.e.	[0.000, 0.657]	0.657	[0.000, 0.657]	0.657
CI	[-0.118, 0.752]		[-0.118, 0.752]	
	MTS + MTR + MIV		**MTS + MTR + MIV**	
	LB UB	width	LB UB	width
p.e	[0.000, 0.657]	0.657	[**0.0310, 0.657**]	0.626
CI	[-0.117, 0.752]		[-0.135, 0.752]	

(B) Expenditure MIV:

	Differential Errors		Nondifferential Errors	
	MTS + MIV		**MTS + MIV**	
	LB UB	width	LB UB	width
p.e.	[-0.485, 0.634]	1.119	[**0.239, 0.634**]	0.394
CI	[-0.685, 0.768]		[**0.006, 0.752**]	
	MTR + MIV		**MTR + MIV**	
	LB UB	width	LB UB	width
p.e.	[0.000, 0.634]	0.634	[0.000, 0.634]	0.634
CI	[-0.164, 0.768]		[-0.164, 0.768]	
	MTS + MTR + MIV		**MTS + MTR + MIV**	
	LB UB	width	LB UB	width
p.e	[0.000, 0.634]	0.634	[**0.242, 0.634**]	0.392
CI	[-0.183, 0.768]		[**0.019, 0.768**]	

3. Data

3.1. Main Data Source

Our main data source is the USDA's National Household Food Acquisition and Purchase Survey (FoodAPS), the first nationally representative survey to collect comprehensive data about household food purchases and acquisitions.[15] The survey was administered on a stratified sample of 4,826 households between April 2012 and January 2013. The FoodAPS sample was drawn from three population groups: SNAP households, low-income households not participating in SNAP, and higher income households. Each household participated in the survey for one seven-day week.

FoodAPS captures detailed information about purchases and acquisitions of food items intended for consumption at home and away from home, including items acquired through USDA's food assistance programs, as well as the amount and source of payment for food. The survey also collects information about household and personal attributes, including conventional demographic and socioeconomic characteristics, health status, diet and nutrition knowledge, non-food expenditures, income, receipt of SNAP benefits (current, last 12 months, and the date of last receipt), confirmation of SNAP receipt through an administrative match, and self-reported WIC receipt along with information to determine WIC eligibility.

Notably, households also filled out a 10-item food security questionnaire (referenced to the last 30 days), which is the basis for calculating raw food security scores and assigning households to categories of food security. Using the USDA's 30-day adult food security scale, "food insecure" households are those with the raw score of 3 or more. Such households can be further categorized as having "low food security" (score of 3-5) or "very low food security" (6-10). Those with scores of 0, 1, or 2 are labeled as "food secure."

Through its Geography Component (FoodAPS-GC), FoodAPS provides information about the local food environments of its participants, including the location of different types of food retailers, measures of access to these retailers, measures of food prices and prices of food categories by retailers, and food-related public policies. We employ FoodAPS-GC to construct variables that can be used as MIVs. In particular, we calculate population-based densities of supermarkets and of all food stores in a household's county of residence. Also, we use food price data to construct

[15] FoodAPS was co-sponsored by ERS and FNS and was conducted in the field by Mathematica Policy Research, a private research firm with experience in large-scale surveys.

measures of the cost of TFP consistent with each household's size and composition. The TFP measures vary with respect to the geographic level of price aggregation, say, county vs. stores located within 20 miles of the household. We then construct a food expenditure MIV by dividing actual household expenditures on food at home by a calculated TFP cost.

For confidentiality reasons, the (complete) FoodAPS data release is restricted-use. We access it through a secure data enclave of the National Opinion Research Center (NORC).[16]

3.2. Supplementary Data Sources

We employ an extract from the SNAP Policy Database (ERS, 2017b) and selected administrative data on SNAP caseloads (namely, the rates of erroneous over- and underpayment of benefits) from FNS covering 2012–2013 to supplement the information in FoodAPS-GC on food-related public policies. Variables constructed from these supplementary data are state- and month-specific and pertain to state-level SNAP policies and design features, such as the magnitude of outreach expenditures, length of recertification periods, exemption of vehicles from the household asset test, reduced reporting requirements, fingerprinting of program applicants, among others. These variables are commonly employed as IVs in the literature studying the effects of SNAP participation (e.g., Gregory and Deb, 2015; Ratcliffe et al., 2011; Yen et al., 2008).

3.3. Analytical Sample

We focus on FoodAPS households that would be eligible to concurrently participate in SNAP and WIC. Given the restrictions associated with these two food programs, our analytical sample is comprised of households with income below 130% of the poverty threshold and containing a pregnant woman, or a child aged less than five years. Our analytical sample includes 460 households. In what follows, all sample statistics and estimates incorporate FoodAPS household weights.

Table 1 provides the joint distribution of the analytical sample by self-reported, current household participation in SNAP and WIC.[17] The table shows that concurrent participation in the two programs is empirically relevant: 36.7% of the households report being on both SNAP and WIC. Also, 16.6% are reportedly on WIC but not SNAP, and 31.4% are on SNAP but not WIC. The remaining 15.3% indicate no participation in either program.

[16] ERS also has created public-use FoodAPS files by purging identifying variables. For details, see ERS (2017a).
[17] In FoodAPS, questions about SNAP and WIC refer to current participation.

To ascertain SNAP participation, Mathematica Policy Research devised an algorithm to match FoodAPS households to SNAP administrative records (namely, SNAP caseload data and ALERT system transactions data). Not all households could be matched, however. Among the households in our analytical sample, 57.6% are matched and confirmed SNAP participants, 2.6% are matched and confirmed nonparticipants, 37.5% could not be matched, and 2.3% withheld consent to be matched. While many unmatched households are likely to be SNAP nonparticipants, we choose to treat all unmatched households (as well as those who withheld consent) as “not verified,” because the failure to match to administrative records may, in part, be due to the specifics of Mathematica’s algorithm and data imperfections, rather than solely due to genuine absence from the records. In a sense, we prefer to be agnostic as to whether unmatched households tell us the truth about their SNAP participation. It should be noted that FoodAPS contains no administrative data on WIC. Thus, all households are “not verified” with respect to WIC participation.

Table 2 is similar to Table 1, except that the SNAP participation indicator now incorporates administrative data in FoodAPS. In particular, for households that are matched to administrative records, SNAP status comes from the administrative record. In all other instances, SNAP participation is self-reported. Compared to the distribution in Table 1, the incorporation of administrative data about SNAP leads to an increase in the prevalence of SNAP participation by about 5 percentage points overall. More specifically, the prevalence of households on SNAP but not on WIC increases by 2.2 percentage points (from 31.4% to 33.6%), while the prevalence of households on both SNAP and WIC rises by 3 percentage points (from 36.7% to 39.7%). Apparently, SNAP participation is underreported in FoodAPS.

Table 3 presents the weighted prevalence of food security in each of four subsamples defined according to self-reported household participation in SNAP and WIC. The rate of food security exceeds 50% throughout, but somewhat varies across the subsamples. (The rate of food security in the analytical sample overall is 55.04%.[18]) Given no WIC receipt, self-reported SNAP participation is associated with a decrease in the prevalence of food security from 53.2% to 52.2%, which is in line with a negative association between food security and SNAP found in the literature (see Gundersen

[18] The corresponding rate of not very low food security (i.e., the absence of very low food security) is 83.21%. Appendix A provides details on the prevalence of not very low food security in the four subsamples defined according to the program participation status.

et al., 2011). When WIC is in place, however, SNAP is associated with an increase in the food security rate from 54.5% to 58.5%. Perhaps the process of selecting into SNAP differs depending on whether the household participates in WIC, or perhaps there are considerable synergies between the two programs in promoting food security. Also, the table shows that self-reported WIC participation is associated with more food security regardless of the self-reported SNAP status.

Table 4, which replaces the self-reported SNAP participation indicator with the administratively matched one, likewise shows food security rates in excess of 50% and varying somewhat across the four subsamples. With one exception, the table indicates similar, if only more pronounced, associations between the participation indicators and food security to those implied by Table 3. The only exception is that given no SNAP receipt, self-reported WIC participation is now associated with less (rather than more) food security. Perhaps when underreported instances of SNAP receipt are removed from the equation, the process of selecting into WIC (as the only program) is actually similar to that of selecting into SNAP (as the only program) in the sense that households with unobservables that are unfavorable to food security are more likely to participate.

Table 5 provides descriptive statistics for selected characteristics of the analytical sample. On average, the sample households contain 4.5 members (of all ages), 2.3 children (aged < 18 years), and 1.6 young children (aged 0–6 years). Average monthly household income is almost $1,607, income-to-poverty ratio is 0.75, and weekly expenditure on food at home is about $113. Twenty-one percent of the households live in rural areas, 78% rent their residence, 26% do not own or lease a vehicle, and 11% have used a food pantry in the past 30 days. Primary respondents in these households are predominantly female (88%) and about 33.7 years old on average. Thirty-three percent are Hispanic, 55% are White, 29% are Black, 32% have no high school degree, 32% have a high school degree or GED, 28% have some college education but no bachelor’s degree, and 7% have a bachelor’s degree. Also, 44% are single (never married), 29% are married, 25% are divorced or separated, and 2% are widowed; 43% are employed, 17% are looking for work, and 40% are not working.

4. Further sensitivity analysis

This section provides an additional set of empirical results under a different set of assumptions about reporting errors. First, we drop the assumption that SNAP verification in FoodAPS is sufficient to identify whether a household belongs to treatment 1 or 3 (SNAP alone or SNAP plus WIC) vs. treatment 0 or 2 (neither program or WIC alone). In exchange, the no false positives assumption is replaced with a stronger assumption that any household willing to acknowledge participation in either SNAP or WIC is a reliable reporter for both programs (not only for the program for which participation is acknowledged). [19]

Figure 1 plots the bounds on the average treatment effect of participating in both SNAP and WIC vs. in SNAP alone, ATE_{31}, when we impose no assumptions on the selection process (i.e., the case of endogenous selection). The values of ATE_{31} are on the vertical axis. On the two horizontal axes, we list possible values of underreporting true participation – specifically, by how many percentage points the true treatment probability exceeds the self-reported rate. The axis on the left shows possible values of underreporting of joint participation in SNAP and WIC. The axis on the right shows analogous values for participation in SNAP alone. The figure utilizes a heat map. The blue surface is the lower bound on ATE_{31}. The yellow surface is the upper bound. We also insert the zero plane to more clearly show that we cannot sign the ATE (without imposing restrictions on the selection process).

Figure 2 shows that, compared to the endogenous selection bounds in Figure 1, the bounds under exogenous selection are very tight. In fact, under no misreporting they collapse to a point above the zero plane (indicated by a small red circle in the figure). Under misreporting, however, the lower bound can still fall below the zero plane.

Table 6 presents point estimates of the bounds under exogenous selection, along with the 95% Imbens and Manski (2004) confidence intervals around them, for selected values of the program participation underreporting.[20] As seen in the table, we are able to sign the ATE as positive for underreporting values of less than 3 percentage points, but we no longer can do so when underreporting reaches 3 percentage points in both directions (even before accounting for the uncertainty associated with the sampling variability of our estimators of the bounds). Moreover, we observe that identification deteriorates rapidly with misreporting. For example, it only takes a one percentage point of underreporting in each direction to result in an expansion of the width of the ATE bounds from 0 to 0.054.

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[19] We are agnostic about the reliability of responses for households that report no participation.
[20] The confidence intervals account for the sampling variability of our estimators of the bounds.

Figure 3 indicates that incorporating the MTS assumption leads to narrower bounds on ATE_{31} compared to the worst-case bounds under endogenous selection (Figure 1). Yet, we are unable to sign the ATE as the lower bound is still below the zero plane.

Figure 4 shows that we can narrow the bounds further by adding the MTR assumption and using an MIV.[21] In fact, if there is very little or a lot of misreporting, it is possible to sign ATE_{31} as strictly positive. However, under moderate amounts of misreporting, the lower bound is zero and we can only claim that the ATE is nonnegative.

5. Conclusion

Low-income households in the United States often receive benefits from more than one food assistance program administered by USDA, which raises the question of how these programs interact in creating a food safety net. We investigate the issue by focusing on two popular programs, SNAP and WIC, and develop a novel nonparametric bounding methodology to handle a multinomial, partially ordered treatment, endogenous selection into assistance programs, and misreporting of program participation (i.e., nonclassical measurement error) in a unifying framework. The literature has shown that even small amounts of misreporting in surveys can lead to much identification decay. However, the availability of validation data may help to offset it and sharpen inferences.

In the empirical analysis, we draw on a unique aspect of FoodAPS in that it provides auxiliary administrative data on SNAP participation, which allows us to partially validate the treatment variable. As is typical of nonparametric bounding analyses, under endogenous selection into the programs and few assumptions, bounds on ATEs are wide and contain zero, which makes it impossible to sign the causal effects. However, we are able to substantially narrow the bounds by combining conventional monotonicity assumptions. We layer successively stronger combinations of assumptions in order to investigate how they shape inference and also provide successively tighter bounds on the causal impact of participation in SNAP and WIC.

Our key finding is that we can identify the ATE as strictly positive and statistically significant under relatively weak assumptions on the selection process combined with a food expenditure monotone instrumental variable. Monotone instrumental variables are weaker than

[21] For this figure, we employ the income-to-poverty MIV. The food expenditure MIVs lead to similar results.

standard IVs in that they require no *a priori* exclusion restriction. We find that participating in both SNAP and WIC compared with participating in SNAP alone is estimated to increase the food security rate among low-income households in our sample by at least 24 percentage points. Accounting for sampling variability reflected in the 95 percent confidence interval, food security would rise by at least 1.9 percentage points. Our empirical results have direct policy relevance in that they inform policy makers about the existence of complementarities (or redundancies) between SNAP and WIC and will help contribute to designing more efficient food assistance programs.

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Tables and Figures

Table 1. Sample Distribution by Reported Program Participation (Weighted)

		WIC	
		No	*Yes*
SNAP	*No*	15.3%	16.6%
	Yes	31.4%	36.7%

Notes: This table provides the joint distribution of the analytical sample (*N* = 460) by self-reported household participation in SNAP and WIC. Observations are weighted using FoodAPS household weights.

Table 2. Sample Distribution by Reported WIC Participation and Administratively Matched SNAP Participation (Weighted)

		WIC	
		No	*Yes*
SNAP	*No*	13.0%	13.6%
	Yes	33.6%	39.7%

Notes: This table provides the joint distribution of the analytical sample (*N* = 460) by household participation in SNAP and WIC. WIC participation is self-reported. SNAP participation incorporates administrative data. In particular, for households that can be matched to administrative records, SNAP participation status reflects the administrative record. For households that cannot be matched, SNAP participation is self-reported. Observations are weighted using FoodAPS household weights.

Table 3. Prevalence of Food Security in Subsamples by Self-Reported Program Participation (Weighted)

SNAP		WIC: *No*	WIC: *Yes*
	No	53.2%	54.5%
	Yes	52.2%	58.5%

Notes: This table shows the prevalence of food security (in percent, weighted) in each of the four subsamples defined according to self-reported participation in SNAP and WIC. Observations are weighted using FoodAPS household weights.

Table 4. Prevalence of Food Security in Subsamples by Self-Reported WIC Participation and Administratively Matched SNAP Participation (Weighted)

SNAP		WIC: *No*	WIC: *Yes*
	No	55.1%	50.5%
	Yes	51.6%	59.5%

Notes: This table shows the prevalence of food security (in percent, weighted) in each of the four subsamples defined according to self-reported participation in WIC and administratively matched participation in SNAP (see the notes to Table 2 for details). Observations are weighted using FoodAPS household weights.

Table 5. Selected Characteristics of Analytical Sample (Weighted)

Characteristic	Mean	Std.Dev.	Min	Max
Household characteristics:				
Number of household members	4.48	1.76	1	≥ 10[a]
Number of children	2.34	1.31	0	≥ 7[a]
Number of children aged 0–6 years	1.57	0.93	0	≥ 5[a]
Household monthly income, $	1,606.69	954.32	0	≥ 4,000[a]
Income-to-poverty ratio	0.75	0.36	0	1.30
Weekly expenditure on food at home, $	112.92	126.00	0	≥ 1,000[a]
Rural household	0.21	0.41	0	1
Household rents its residence	0.78	0.41	0	1
No household member owns or leases a vehicle	0.26	0.44	0	1
Household has used food pantry (past 30 days)	0.11	0.31	0	1
Primary respondent's characteristics:				
Female	0.88	0.32	0	1
Age, years	33.71	10.75	17	≥ 70[a]
Hispanic (ethnicity)	0.33	0.47	0	1
White (race)	0.55	0.50	0	1
Black (race)	0.29	0.45	0	1
All other races	0.16	0.37	0	1
Less than high school degree	0.32	0.47	0	1
High school degree	0.24	0.43	0	1
GED	0.08	0.27	0	1
Some college education	0.20	0.40	0	1
Associate's degree	0.08	0.27	0	1
Bachelor's or higher degree	0.07	0.26	0	1
Single (never married)	0.44	0.50	0	1
Married	0.29	0.45	0	1
Divorced	0.17	0.38	0	1
Separated	0.08	0.27	0	1
Widowed	0.02	0.17	0	1
Employed	0.43	0.50	0	1
Looking for work	0.17	0.38	0	1
Not working	0.40	0.49	0	1

Note: This table shows descriptive statistics for selected characteristics of the analytical sample. Observations are weighted using FoodAPS household weights.
[a] An exact maximum value is suppressed due to confidentiality concerns.

Table 6. Identification Decay due to Misreporting under Exogenous Selection

SNAP:		$\Delta_1 = 0$		$\Delta_1 = 0.01$		$\Delta_1 = 0.03$	
both:		LB UB	width	LB UB	width	LB UB	width
$\Delta_3 = 0$	p.e.	[0.070, 0.070]	0.000	[0.056, 0.085]	0.029	[0.030, 0.113]	0.083
	CI	[-0.066, 0.206]		[-0.065, 0.206]		[-0.074, 0.217]	
$\Delta_3 = 0.01$	p.e.	[0.056, 0.080]	0.024	[0.042, 0.095]	0.054	[0.016, 0.123]	0.107
	CI	[-0.068, 0.204]		[-0.071, 0.208]		[-0.085, 0.224]	
$\Delta_3 = 0.03$	p.e.	[0.029, 0.099]	0.069	[0.015, 0.114]	0.099	[-0.011, 0.142]	0.152
	CI	[-0.081, 0.208]		[-0.089, 0.217]		[-0.109, 0.239]	

Note: This table presents estimates of the bounds on ATE_{31} under exogenous selection for selected values of program participation underreporting. “LB” and “UB” stand for lower and upper bounds, respectively. “P.e.” refers to a point estimate. “CI” represents the Imbens and Manski (2004) 95% confidence interval around the bounds.

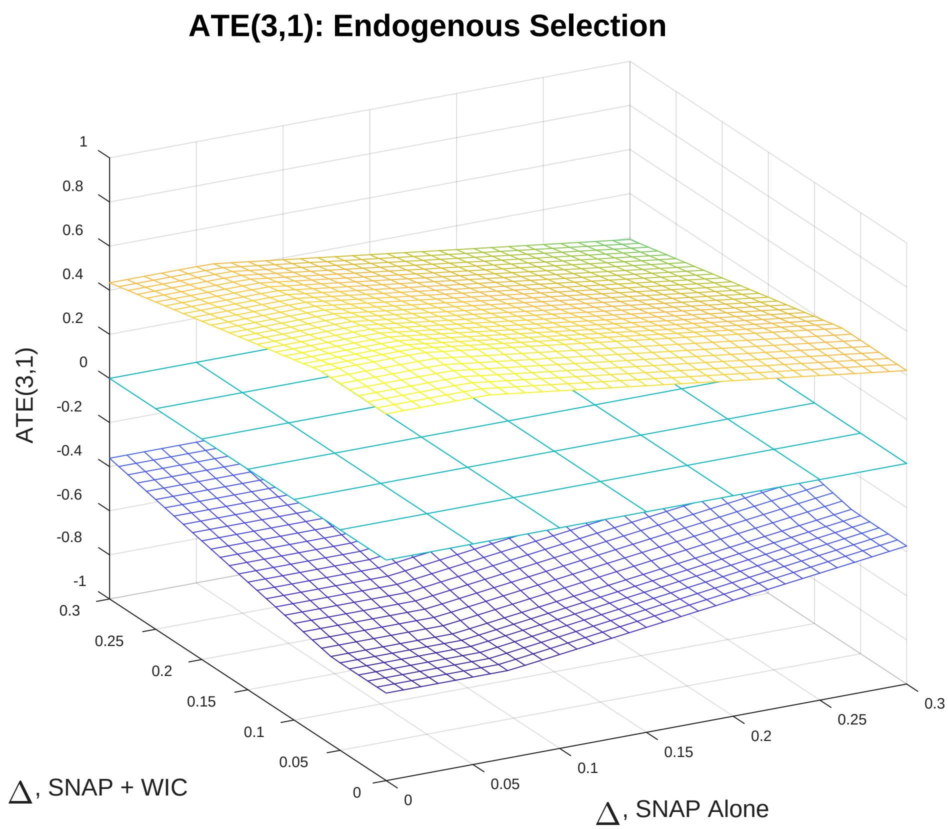

Figure 1. Bounds on ATE_{31} under Endogenous Selection

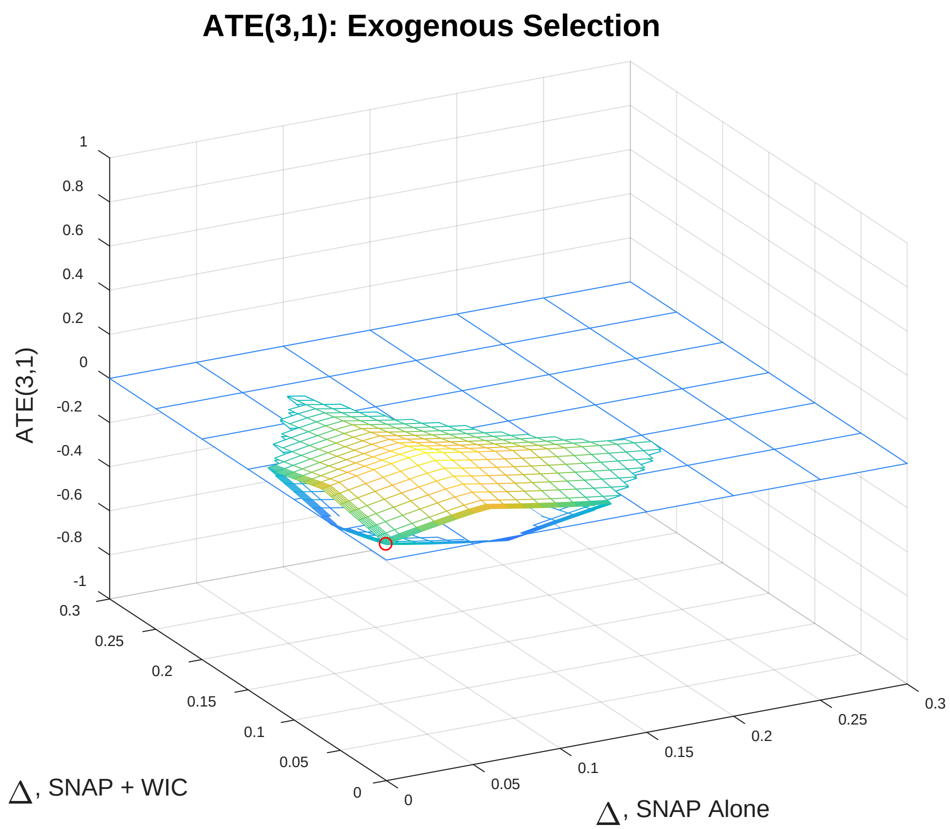

Figure 2. Bounds on ATE_{31} under Exogenous Selection

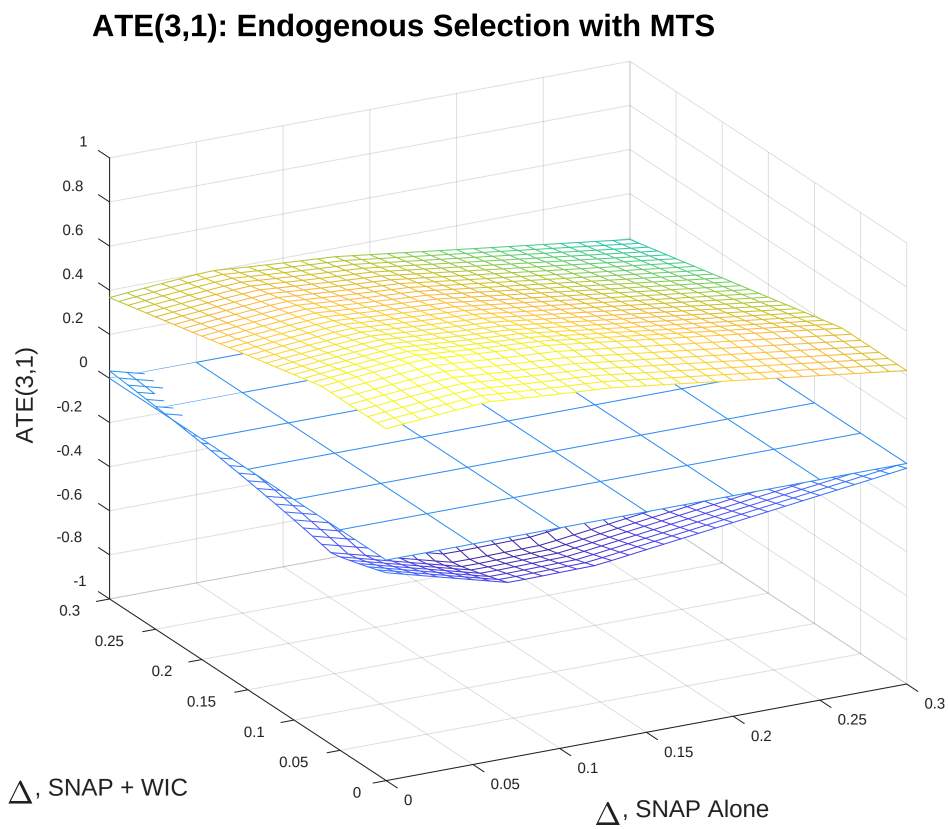

Figure 3. Bounds on ATE_{31} under Endogenous Selection with MTS Assumption

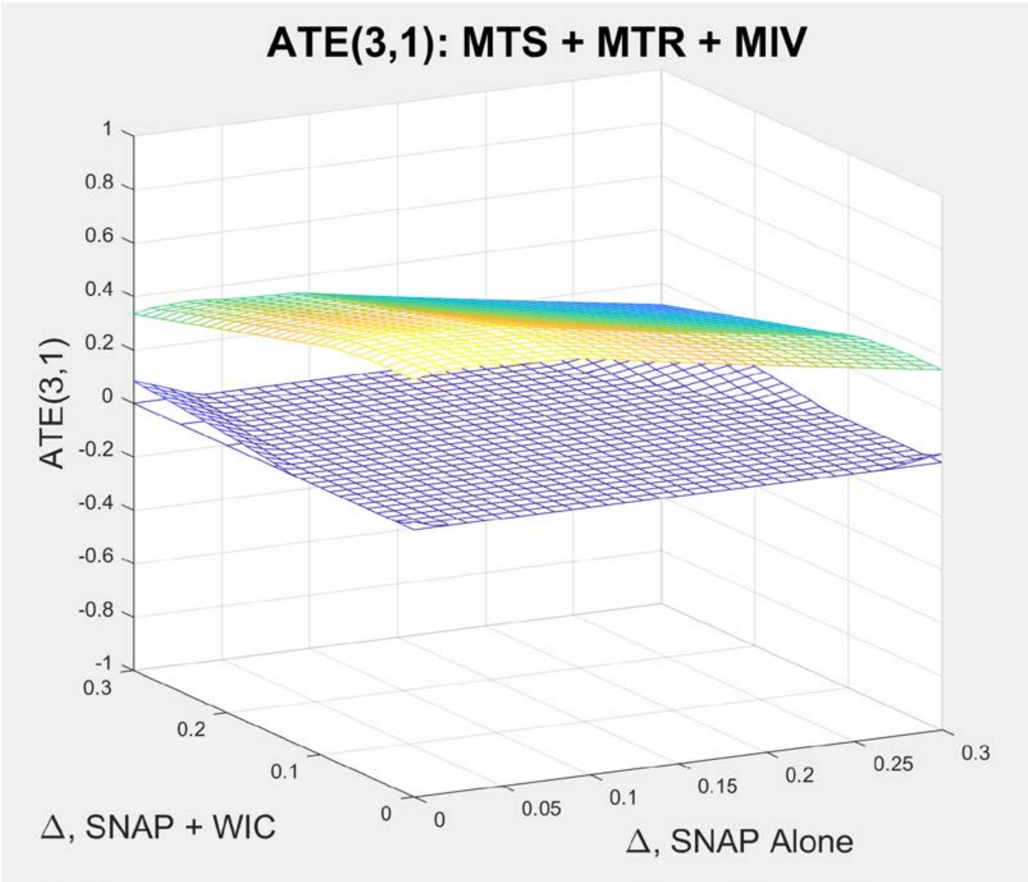

Figure 4. Bounds on ATE_{31} under Endogenous Selection with MTS, MTR, and MIV

Appendix A

Table A1. Prevalence of Not Very Low Food Security in Subsamples by Self-Reported Program Participation (Weighted)

		WIC	
		No	*Yes*
SNAP	*No*	74.6%	83.7%
	Yes	78.7%	90.4%

Notes: This table shows the prevalence of not very low food security (in percent, weighted) in each of the four subsamples defined according to self-reported participation in SNAP and WIC. Observations are weighted using FoodAPS household weights.

Table A2. Prevalence of Not Very Low Food Security in Subsamples by Self-Reported WIC Participation and Administratively Matched SNAP Participation (Weighted)

		WIC	
		No	*Yes*
SNAP	*No*	73.4%	81.3%
	Yes	78.9%	90.7%

Notes: This table shows the prevalence of not very low food security (in percent, weighted) in each of the four subsamples defined according to self-reported participation in WIC and administratively matched participation in SNAP (see the notes to Table 2 for details). Observations are weighted using FoodAPS household weights.

Proofs of propositions:

Proposition 1A. Worst-case $ATE_{3,1}$ bounds under FoodAPS validation:

Lower bound: Set $\theta_1^{1,3} = 0$ and $\theta_0^{1,3} = P(Y=0, S=1),$ its largest feasible value, to obtain

$LB = -P(Y=0, S \neq 1) - P(Y=1, S \neq 3) - P(Y=0, S=1).$ We can then simplify as follows:

$LB = -P(Y=0) - P(Y=1, S \neq 3) = -1 + P(Y=1) - P(Y=1, S \neq 3) = -1 + P(Y=1, S=3).$

Upper bound: Set $\theta_0^{1,3} = 0$ and $\theta_1^{1,3} = P(Y=1, S=1),$ its largest feasible value, to obtain

$UB = P(Y=0, S \neq 3) + P(Y=1, S \neq 1) + P(Y=1, S=1).$ We can then simplify as follows:

$UB = P(Y=0, S \neq 3) + P(Y=1) = P(Y=0, S \neq 3) + 1 - P(Y=0) = 1 - P(Y=0, S=3).$

Proposition 1B. Worst-case $ATE_{3,1}$ bounds under FoodAPS validation with nondifferential errors:

Lower bound: If $\kappa > 1,$ set $\theta_1^{1,3} = P(Y=1, S=1),$ its largest feasible value. If $\kappa \leq 1,$ set $\theta_1^{1,3} = 0.$ Then the lower bound is given by

$$LB = \begin{cases} -1 + P(Y=1, S=3) + P(Y=0, S=1) & \text{if } \kappa \leq 1 \Leftrightarrow P(Y=0, S=1) \leq P(Y=1, S=1) \\ -1 + P(Y=1, S=3) + P(Y=0, S=1) & \\ \quad + \left[\dfrac{P(Y=1, S=1) - P(Y=0, S=1)}{P(Y=1, S=1)} \right] P(Y=1, S=1) & \text{if } \kappa > 1 \Leftrightarrow P(Y=1, S=1) < P(Y=0, S=1) \end{cases}$$

or

$$LB = \begin{cases} -1 + P(Y=1, S=3) + P(Y=0, S=1) & \text{if } \kappa \leq 1 \Leftrightarrow P(Y=0, S=1) \leq P(Y=1, S=1) \\ -1 + P(Y=1, S=3) + P(Y=1, S=1) & \text{if } \kappa > 1 \Leftrightarrow P(Y=1, S=1) < P(Y=0, S=1). \end{cases}$$

Rewriting, $LB = -1 + P(Y=1, S=3) + \min\{P(Y=0, S=1), P(Y=1, S=1)\}.$ □

Upper bound: If $\kappa < 1,$ set $\theta_1^{1,3} = P(Y=1, S=1),$ its largest feasible value. If $\kappa \geq 1,$ set $\theta_1^{1,3} = 0.$ Then the upper bound is given by

$$UB = \begin{cases} 1 - P(Y=0, S=3) - P(Y=1, S=1) & \text{if } \kappa \geq 1 \Leftrightarrow P(Y=1, S=1) \leq P(Y=0, S=1) \\ 1 - P(Y=0, S=3) - P(Y=1, S=1) \\ \quad + \left[\dfrac{P(Y=1, S=1) - P(Y=0, S=1)}{P(Y=1, S=1)} \right] P(Y=1, S=1) & \text{if } \kappa < 1 \Leftrightarrow P(Y=0, S=1) < P(Y=1, S=1) \end{cases}$$

or

$$UB = \begin{cases} 1 - P(Y=0, S=3) - P(Y=1, S=1) & \text{if } \kappa \geq 1 \Leftrightarrow P(Y=1, S=1) \leq P(Y=0, S=1) \\ 1 - P(Y=0, S=3) - P(Y=0, S=1) & \text{if } \kappa < 1 \Leftrightarrow P(Y=0, S=1) < P(Y=1, S=1). \end{cases}$$

Rewriting, $UB = 1 - P(Y=0, S=3) - \min\{P(Y=0, S=1), P(Y=1, S=1)\}$. □

Proposition 2A. Exogenous selection:

Under exogenous selection, we can write $P[Y(3)=1] = P[Y(3)=1 \mid S^*=3] = P(Y=1 \mid S^*=3)$

$= \dfrac{P(Y=1, S=3) + \theta_1^{-3,3} - \theta_1^{3,-3}}{P(S=3) + \theta_1^{-3,3} + \theta_0^{-3,3} - \theta_1^{3,-3} - \theta_0^{3,-3}}$. Similarly, we can write $P[Y(1)=1] = P[Y(1)=1 \mid S^*=1]$

$= P(Y=1 \mid S^*=1) = \dfrac{P(Y=1, S=1) + \theta_1^{-1,1} - \theta_1^{1,-1}}{P(S=1) + \theta_1^{-1,1} + \theta_0^{-1,1} - \theta_1^{1,-1} - \theta_0^{1,-1}}$. Under no false positives, we have

$P[Y(3)=1] = \dfrac{P(Y=1, S=3) + \theta_1^{1,3}}{P(S=3) + \theta_1^{1,3} + \theta_0^{1,3}}$ and $P[Y(1)=1] = \dfrac{P(Y=1, S=1) - \theta_1^{1,3}}{P(S=1) - \theta_1^{1,3} - \theta_0^{1,3}}$.

For the lower bound, set $\theta_1^{1,3} = 0$ and $\theta_0^{1,3} = P(Y=0, S=1)$. Then

$$\begin{aligned} ATE_{3,1} &\geq \frac{P(Y=1, S=3)}{P(S=3) + P(Y=0, S=1)} - \frac{P(Y=1, S=1)}{P(S=1) - P(Y=0, S=1)} \\ &= \frac{P(Y=1, S=3)}{P(S=3) + P(Y=0, S=1)} - \frac{P(Y=1, S=1)}{P(Y=1, S=1)} = \frac{P(Y=1, S=3)}{P(S=3) + P(Y=0, S=1)} - \frac{P(Y=1, S=1)}{P(Y=1, S=1)} \\ &= \frac{P(Y=1, S=3)}{P(S=3) + P(Y=0, S=1)} - 1 = \frac{P(Y=1, S=3) - P(S=3) - P(Y=0, S=1)}{P(S=3) + P(Y=0, S=1)} \\ &= \frac{-P(Y=0, S=3) - P(Y=0, S=1)}{P(S=3) + P(Y=0, S=1)}. \end{aligned}$$

For the upper bound, set $\theta_1^{1,3} = P(Y=1, S=1)$ and $\theta_0^{1,3} = 0$. Then

$$ATE_{3,1} \le \frac{P(Y=1,S=3)+P(Y=1,S=1)}{P(S=3)+P(Y=1,S=1)}. \quad \square$$

Proposition 2B. Exogenous selection and nondifferential errors:

From the preceding proof, the average treatment effect is given by

$ATE_{3,1} = \frac{P(Y=1,S=3)+\theta_1^{1,3}}{P(S=3)+\theta_1^{1,3}+\theta_0^{1,3}} - \frac{P(Y=1,S=1)-\theta_1^{1,3}}{P(S=1)-\theta_1^{1,3}-\theta_0^{1,3}}$. Under nondifferential errors, set $\theta_0^{1,3} = \kappa\theta_1^{1,3}$

such that $ATE_{3,1} = \frac{P(Y=1,S=3)+\theta_1^{1,3}}{P(S=3)+(1+k)\theta_1^{1,3}} - \frac{P(Y=1,S=1)-\theta_1^{1,3}}{P(S=1)-(1+k)\theta_1^{1,3}}$.

For the lower bound, set $\theta_1^{1,3} = 0$. Then $ATE_{3,1} \ge \frac{P(Y=1,S=3)}{P(S=3)} - \frac{P(Y=1,S=1)}{P(S=1)}$

$= P(Y=1 \mid S=3) - P(Y=1 \mid S=1)$.

For the upper bound, set $\theta_1^{1,3} = P(Y=1,S=1)$. Using $\kappa = \frac{P(Y=0,S=1)}{P(Y=1,S=1)}$, we have

$$ATE_{3,1} = \frac{P(Y=1,S=3)+P(Y=1,S=1)}{P(S=3)+P(Y=1,S=1)+kP(Y=1,S=1)} = \frac{P(Y=1,S=3)+P(Y=1,S=1)}{P(S=3)+P(S=1)}. \quad \square$$

Proposition 3A. *$ATE_{3,1}$* LB under FoodAPS validation and MTS:

Using the law of total probability,

$$\begin{aligned}
ATE_{3,1} &= P[Y(3)=1] - P[Y(1)=1] \\
&= \left\{P[Y(3)=1 \mid S^*=3]P_3^* + P[Y(3)=1 \mid S^*=2]P_2^* + P[Y(3)=1 \mid S^*=1]P_1^* + P[Y(3)=1 \mid S^*=0]P_0^*\right\} \\
&\quad -\left\{P[Y(1)=1 \mid S^*=3]P_3^* + P[Y(1)=1 \mid S^*=2]P_2^* + P[Y(1)=1 \mid S^*=1]P_1^* + P[Y(1)=1 \mid S^*=0]P_0^*\right\} \\
&\ge \left\{P[Y(3)=1 \mid S^*=3]P_3^* + P[Y(3)=1 \mid S^*=3]P_2^* + P[Y(3)=1 \mid S^*=3]P_1^* + P[Y(3)=1 \mid S^*=3]P_0^*\right\} \\
&\quad -\left\{P[Y(1)=1 \mid S^*=1]P_3^* + P_2^* + P[Y(1)=1 \mid S^*=1]P_1^* + P_0^*\right\} \\
&= \left\{P(Y=1 \mid S^*=3)\right\} - \left\{P(Y=1 \mid S^*=1)P_3^* + P(Y=1 \mid S^*=1)P_1^* + 1 - P_3^* - P_1^*\right\}
\end{aligned}$$

where the inequality follows from the MTS assumption. Rewriting, we have

$$ATE_{3,1} \geq \frac{P(Y=1,S^*=3)}{P(S^*=3)} - \left\{ \frac{P(Y=1,S^*=1)}{P(S^*=1)}(P_3^*+P_1^*)+1-(P_3^*+P_1^*) \right\}$$

$$= \frac{P(Y=1,S^*=3)}{P(S^*=3)} - \left\{ \left[\frac{P(Y=1,S^*=1)}{P(S^*=1)} - 1 \right](P_3^*+P_1^*)+1 \right\}$$

$$= \frac{P(Y=1,S^*=3)}{P(S^*=3)} - \left\{ \left[\frac{P(Y=1,S^*=1)-P(S^*=1)}{P(S^*=1)} \right](P_3^*+P_1^*)+1 \right\}$$

$$= \frac{P(Y=1,S^*=3)}{P(S^*=3)} - \left\{ 1 - \frac{P(Y=0,S^*=1)}{P(S^*=1)}(P_3^*+P_1^*) \right\}$$

$$= -1 + \frac{P(Y=1,S^*=3)}{P(S^*=3)} + \frac{P(Y=0,S^*=1)}{P(S^*=1)}(P_3^*+P_1^*).$$

Decomposing into observed and unobserved components,

$$ATE_{3,1} \geq -1 + \frac{P(Y=1,S=3)+\theta_1^{-3,3}-\theta_1^{3,-3}}{P(S=3)+\theta_1^{-3,3}+\theta_0^{-3,3}-\theta_1^{3,-3}-\theta_0^{3,-3}} + \frac{P(Y=0,S=1)+\theta_0^{-1,1}-\theta_0^{1,-1}}{P(S=1)+\theta_1^{-1,1}+\theta_0^{-1,1}-\theta_1^{1,-1}-\theta_0^{1,-1}}$$
$$\times\left[P(S=3)+P(S=1)+\theta_1^{-3,3}+\theta_0^{-3,3}-\theta_1^{3,-3}-\theta_0^{3,-3}+\theta_1^{-1,1}+\theta_0^{-1,1}-\theta_1^{1,-1}-\theta_0^{1,-1}\right]$$

$$= -1 + \frac{P(Y=1,S=3)+\theta_1^{1,3}-\theta_1^{3,1}}{P(S=3)+\theta_1^{1,3}+\theta_0^{1,3}-\theta_1^{3,1}-\theta_0^{3,1}} + \frac{P(Y=0,S=1)+\theta_0^{3,1}-\theta_0^{1,3}}{P(S=1)+\theta_1^{3,1}+\theta_0^{3,1}-\theta_1^{1,3}-\theta_0^{1,3}}$$
$$\times\left[P(S=3)+P(S=1)+\theta_1^{1,3}+\theta_0^{1,3}-\theta_1^{3,1}-\theta_0^{3,1}+\theta_1^{3,1}+\theta_0^{3,1}-\theta_1^{1,3}-\theta_0^{1,3}\right].$$

$$= -1 + \frac{P(Y=1,S=3)+\theta_1^{1,3}-\theta_1^{3,1}}{P(S=3)+\theta_1^{1,3}+\theta_0^{1,3}-\theta_1^{3,1}-\theta_0^{3,1}} + \frac{P(Y=0,S=1)+\theta_0^{3,1}-\theta_0^{1,3}}{P(S=1)+\theta_1^{3,1}+\theta_0^{3,1}-\theta_1^{1,3}-\theta_0^{1,3}}\left[P(S=3)+P(S=1)\right].$$

Under no false positives,

$$ATE_{3,1} \geq -1 + \frac{P(Y=1,S=3)+\theta_1^{1,3}}{P(S=3)+\theta_1^{1,3}+\theta_0^{1,3}} + \frac{P(Y=0,S=1)-\theta_0^{1,3}}{P(S=1)-\theta_1^{1,3}-\theta_0^{1,3}}\left[P(S=3)+P(S=1)\right]. \quad \text{(A1)}$$

Recall that $\theta_1^{1,3} \in [0, P(Y=1,S=1)]$ and $\theta_0^{1,3} \in [0, P(Y=0,S=1)]$. To minimize the preceding expression, set $\theta_1^{1,3} = 0$ and $\theta_1^{1,3} = P(Y=1, S=1)$. Hence, the $ATE_{3,1}$ lower bound is given by

$$ATE_{3,1}^{MTS,LB} = -1 + \frac{P(Y=1, S=3)}{P(S=3) + P(Y=0, S=1)}.$$

Using the same approach, it can be shown that the upper bound is not improved. □

Proposition 3B. $ATE_{3,1}$ LB under FoodAPS validation and MTS with nondifferential errors:

Under the nondifferential errors assumption, set $\theta_0^{1,3} = \kappa\theta_1^{1,3}$ in Equation (A1) above to obtain

$$ATE_{3,1}^{MTS} \geq -1 + \frac{P(Y=1, S=3) + \theta_1^{1,3}}{P(S=3) + (1+\kappa)\theta_1^{1,3}} + \frac{P(Y=0, S=1) - \kappa\theta_1^{1,3}}{P(S=1) - (1+\kappa)\theta_1^{1,3}}\left[P(S=3) + P(S=1)\right]$$

where $\kappa \equiv P(Y=0, S=1) / P(Y=0, S=1)$. First, it is straightforward to show that

$\dfrac{P(Y=0, S=1) - \kappa\theta_1^{1,3}}{P(S=1) - (1+\kappa)\theta_1^{1,3}} = \dfrac{P(Y=0, S=1)}{P(S=1)} = P(Y=0 \mid S=1).$ Next, the derivative of

$\dfrac{P(Y=1, S=3) + \theta_1^{1,3}}{P(S=3) + (1+\kappa)\theta_1^{1,3}}$ with respect to $\theta_1^{1,3}$ has the same sign as $P(Y=3) - (1+\kappa)P(Y=1, S=3)$

$= P(S=3) - \dfrac{P(S=1)}{P(Y=1, S=1)}P(Y=1, S=3),$ which in turn has the same sign as

$= \dfrac{P(Y=1, S=1)}{P(S=1)} - \dfrac{P(Y=1, S=3)}{P(S=3)}.$ For the lower bound, we therefore set

$$\theta_1^{1,3} = \begin{cases} 0 & \text{if } \dfrac{P(Y=1, S=3)}{P(S=3)} \geq \dfrac{P(Y=1, S=1)}{P(S=1)} \\[2ex] P(Y=1, S=1) & \text{if } \dfrac{P(Y=1, S=3)}{P(S=3)} < \dfrac{P(Y=1, S=1)}{P(S=1)}, \end{cases}$$

or $$\frac{P(Y=1, S=3) + \theta_1^{1,3}}{P(S=3) + (1+\kappa)\theta_1^{1,3}} = \begin{cases} \dfrac{P(Y=1, S=3)}{P(S=3)} & \text{if } \dfrac{P(Y=1, S=3)}{P(S=3)} \geq \dfrac{P(Y=1, S=1)}{P(S=1)} \\[2ex] \dfrac{P(Y=1, S=3) + P(Y=1, S=1)}{P(S=3) + P(S=1)} & \text{if } \dfrac{P(Y=1, S=3)}{P(S=3)} < \dfrac{P(Y=1, S=1)}{P(S=1)}. \end{cases}$$

It is straightforward to show that $\dfrac{P(Y=1, S=3)}{P(S=3)} \geq \dfrac{P(Y=1, S=3) + P(Y=1, S=1)}{P(S=3) + P(S=1)}$ is equivalent to

$\frac{P(Y=1,S=3)}{P(S=3)} \geq \frac{P(Y=1,S=1)}{P(S=1)}$. Therefore, we set $\frac{P(Y=1,S=3)+\theta_1^{1,3}}{P(S=3)+(1+\kappa)\theta_1^{1,3}}$ equal to

$\max\left\{\frac{P(Y=1,S=3)}{P(S=3)}, \frac{P(Y=1,S=3)+P(Y=1,S=1)}{P(S=3)+P(S=1)}\right\}$.

Using the same approach, it can be shown that the upper bound is not improved. □